

ACADEMIC ROOTS: COLUMBIA UNIVERSITY, 1943-1951

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It is now an even hundred years since the establishment by James McKeen Cattell of the first psychological laboratory at Columbia University and 40 years since Fred Keller and Nat Schoenfeld's *Principles of Psychology: A Systematic Text* (1950) first appeared in print (for reviews, see Brown, 1952; Dinsmoor, 1989). It therefore seems like a fitting occasion on which to describe what life was like in the Columbia department just before and just after Keller and Schoenfeld launched their innovative and influential experiment in education at Columbia College, the university's division for undergraduate males. (A similar program for undergraduate females was instituted at Mount Holyoke the same year, and at least some aspects of the program were copied at a number of other institutions.)

I first entered Columbia in June of 1943 by the simple expedient of walking into the graduate admissions office and registering for summer courses in intelligence testing and in applied psychology. Perhaps I didn't state my ultimate objective. No one said anything about seeking the approval of the department, but fortunately the tradition there was to let almost anyone in for the first year and then to winnow out the doctoral candidates. A comprehensive examination was given, with questions written by six members of the senior faculty: Henry E. Garrett in Tests and Measurements, Otto Klineberg in Social, Carney Landis in Abnormal, A. T. Poffenberger in Applied, C. J. Warden in Comparative, and Robert S. Woodworth in Experimental. (As Gardner Murphy had left Columbia and Fred Keller was at Camp Crowder, there was no

section on History that year.) Evidently I did well enough on the exam and in my classes to justify keeping me on.

Physical Layout

The Department of Psychology occupied the first three and a half floors of a wing that had been added thirteen years before I came there; it was an extension to the original structure of Schermerhorn Hall and was therefore known as Schermerhorn Extension. The main part of the campus lay between Broadway on the west and Amsterdam on the east, extending for a number of blocks north and south. There were no cross streets cutting through between 116th St., where commuting students emerged from the Broadway branch of the IRT subway, and 120th St., where Teachers College began. On Amsterdam, there was an entrance gate that led to the first floor of the extension building and to a small park-like area at the corner of 120th St. There in sunny weather neighborhood mothers congregated with their carriages and their strollers. But Amsterdam Avenue rose at a fairly steep slope and in addition most of the central section of the campus was elevated well above the street level, with the result that after crossing from the subway entrance one could continue directly into the fourth floor of Schermerhorn Extension from the first floor of the main building. It was at this intersection that our mailboxes were located. The departmental office was nearby.

The first floor of the extension building was readily accessible to the public and was devoted entirely to rooms in which classes met; there were additional classrooms on the other floors, but there were also offices and laboratories.

The second floor was dedicated to animal work. There one could find the offices of C. J. Warden and Fred S. Keller, some rooms used for research purposes, Room 252—which

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was eventually to be remodeled as a laboratory classroom for Keller and Schoenfeld's introductory course—and at the northern (uptown) end of the corridor, the animal quarters, known as the "vivarium." When I first arrived on the scene, the vivarium was largely occupied by colonies of cats and monkeys. These animals were maintained by C. J. Warden for studies in comparative psychology, a subject in which I had not the slightest interest. After the new introductory course was established for the College, however, the area was gradually taken over by the white rats and the people who worked with them. The animal caretaker throughout the time I was there was a small, middle-aged Irishman named Mike Riordan. Mike was always helpful with such information as how to pick up and hold a rat, what the regulations were for obtaining and maintaining experimental subjects, how to treat the sniffles, where supplies were kept, and how to dispose of animals that were no longer needed. The graduate students were fond of Mike, and I remember looking him up when I came back to the building for a visit some years later. The vivarium was a place where one often crossed paths with other rat runners and stopped for a few minutes of conversation. Along with two or three of the offices housing graduate students, it served as one of the social centers for the department. During the last part of my stay at Columbia a few of us, including Nat Schoenfeld, even used to lunch together in one of the back rooms.

The third floor was dedicated primarily to human research, under the supervision of men like R. S. Woodworth and later Clarence H. Graham, but Nat Schoenfeld's and Ralph Hefferline's offices were also there. So were the machine shop and the room where the departmental seminar met.

The fourth floor was shared with a small but extremely distinguished Department of Anthropology (Boas, Mead, and Welfish come to mind). It included the administrative suite, Otto Klineberg's office, and a room full of calculating machines. The departmental chair throughout my stay was Henry Garrett, a psychometrician who later testified before the Supreme Court in opposition to the desegregation of educational facilities in the southern states. We did not share his views.

The departmental secretary was Louise Miller, and I soon came to realize that she was a key person for a graduate student to know.

The fifth to the eighth floors of the building were *terra incognita*, on which I hardly ever set foot, but two elevators with uniformed operators on duty during business hours were available to take us up to the ninth floor, where one of the department's most important resources, the Psychology Library, was located. The room was spacious and well lighted, with a number of large tables where students could prepare assigned papers or oral reports, search the literature for the background to a dissertation, or simply browse through the current journals. All of the psychological journals were shelved in this library, in alphabetical order, and were readily accessible from where we were working. There was also an excellent collection of books: It was a rare event when I did not find whatever volume I might be looking for. The head librarian was a gracious, supportive, and helpful woman named Enrica Tunnell. I suspect that Mrs. T contributed much more than most of us realized to the scholarship of the period. She was a widow, and legend had it that she resided with R. S. Woodworth, whose wife was said to be confined to a mental institution.

Along the western wall of the library was a file of small cards, unique to Columbia, that gave the citations for all the psychological articles for which a given individual was listed as the initial author. If you knew the names of the authors, you could locate the article in this file. If you wanted to read further work published under the same name, it was also listed there. Some years later this file was published under the title of *Author Index to Psychological Index 1894-1935 and Psychological Abstracts 1927-1958* (Columbia University, 1960) and is available in a number of major libraries. Very few people are familiar with this set of volumes, but it is an extremely useful entree into the early literature. In the preface, the Director of Libraries at Columbia at the time of its publication noted that "Its principal supporters in the immediate past have been Professor Robert S. Woodworth, now Emeritus, and Mrs. Enrica Tunnell, who was the Psychology Librarian for more than thirty years. To them is owed a special debt of gratitude."

Secret Project

It was war time, and as I crossed the campus to and from the subway station I often passed groups of men in the uniform of the Naval ROTC marching in formation down the brick-paved pathways between the buildings. As an internationalist steeped in the traditions of Eugene Debs, Keir Hardie, Jean Jaures, Karl Liebknecht, and Rosa Luxemburg, the sight made me feel uneasy, but I held my peace. I would have been still more uneasy had I guessed what was taking place in the secret project on the other side of the wall between our building and the basement of Schermerhorn. With proper credentials one could have entered the area simply by continuing down the corridor from the third floor of the extension, but armed guards barred the way. It was part of the Manhattan Project, and it was only after the U.S. had dropped atomic bombs on the Japanese cities of Hiroshima and Nagasaki that the secrecy was lifted and I learned of its mission.

A Scientific Education

Without a doubt, Columbia was one of the world's outstanding departments. In the period up to 1948 (my degree was dated 1949), it had awarded 344 doctorates, 75 more than any other department in the country (Harper, 1949). The quality was also high. In subsequent years, a number of my contemporaries have shown up on lists of those heading various divisions of the American Psychological Association or as members of its Council of Representatives. A survey conducted almost two decades after I left showed that of the heads of departments appointed between 1947 and 1970 more had been trained at Columbia than at any other institution (Heckel, 1972). Although many of its students went on to become practitioners in clinical or industrial settings, the department stressed its commitment to research and to the scientific tradition in psychology. While I was there, for example, it succeeded in transferring its affiliation within the university administrative structure to the Faculty of Pure Science.

One of the courses I took in my first year was labeled Experiments in Abnormal Psychology. In that course, we tested resident patients at the New York State Psychiatric

Institute and reported on the results, relating them to the research literature. That was not a true experimental course, to be sure, but it showed that Carney Landis and Joe Zubin, the faculty in that area, took science seriously. I also took a course in Intermediate Experimental Psychology with Nat Schoenfeld. In my second year I took a course entitled Advanced Experimental Psychology (Psychology 203-204), which I believe was required of all PhD candidates. Working in pairs, each of the students was required to design, carry out, and write up a series of original experiments. It was the last year in which the course was taught by the department's most eminent figure, Robert S. Woodworth. Affectionately known among the students as "Woody," Woodworth represented a personal link to the early history of scientific psychology in this country: He had earned his doctorate in 1899, a year after E. L. Thorndike, and shortly afterward had been the coauthor with Thorndike of a classic paper that was widely cited in introductory texts for its attack on the doctrine of formal discipline (Thorndike & Woodworth, 1901). His own introductory textbook (e.g., Woodworth, 1944) had been translated into Arabic, Bengali, Finnish, French, Japanese, Spanish, and Turkish. He had also written the standard, authoritative textbook in experimental psychology (Woodworth, 1938) and a highly regarded survey of the different schools of thought within the discipline (Woodworth, 1931).

During the fall semester of 1944-1945, there were only four students in Advanced Experimental. My lab partner was Donald Gordon. We took up the psychophysical methods and the individual senses, including taste, touch, smell, and the cutaneous senses, and I must admit that I considered the work fairly dull. During the second semester my lab partner was Katherine Pease, who was a doctoral candidate at Teachers College, on the other side of 120th St. That semester we dealt with more complex matters like perception and the various forms of learning experiment. These topics related more closely to my interests in clinical and social psychology, but by then my first child had been born. I was up several times a night, and I had difficulty staying awake during the lectures. Despite that problem, I was greatly

impressed by Woody's seemingly inexhaustible knowledge of the subject matter. And even though I could not complete my experimental reports before the end of the semester and had to take an Incomplete in the course, I made an enlightening if frustrating discovery: I could not devise an empirical test for a single one of the principles of perceptual organization promulgated by the Gestalt psychologists that clearly distinguished the outcome from what might have been predicted from other points of view. That failure made a lasting impression, because it seemed to me that if these principles never led to distinctive predictions they could not be very useful. By contrast, the relative specificity and testability of the work on conditioning did much to convince me that the animal laboratory might provide the key to the broader mysteries of psychology.

During the regular academic year, the doctoral candidates and the faculty met once a week at what was known as the Departmental Seminar (see Woodworth, 1942). Each of the candidates for the degree eventually arranged a date for a presentation and review of his or her proposed doctoral research at one of these meetings (see Figure 4). In the early years, before the number became too large, a full hour was devoted to every candidate. Later, at a second meeting, the candidate's results and conclusions were subjected to a similar scrutiny. Suggestions and criticisms were offered by students and faculty alike, in a spirit of scientific egalitarianism. It was understood that merit, and not rank, was the criterion by which a contribution was to be evaluated. In those days, psychology was considered a single discipline, and almost everyone participated in the discussion, without undue regard to area of specialization. Perhaps I was biased, but in the later years it seemed to me that it was the students in conditioning that typically asked the most probing questions and offered the most useful suggestions. The process was of immense educational value, as differential reinforcement of our contributions by peer approval served to hone our skills in designing and interpreting psychological research. The discussions in the seminar also built our confidence by showing that we were capable of dealing with these issues at a level comparable (if not superior!) to that of some of the best known names in the field.

I Learn About Skinner

Skinner was not well known at the time. I had run across his name in Woodworth's *Experimental Psychology* (1938), which we had used as an undergraduate text at Dartmouth. But Woodworth had given him little more than half a page, under the heading of procedures in which the conditional response produced the unconditional stimulus. That sounded a bit odd to me. Although Woodworth did see "the Skinner experiment [as bridging] the gap between the more usual experiment in conditioning and the puzzle-box experiment" (p. 107), he treated bar pressing as an investigatory response, and he did not mention any of Skinner's work on more complex processes like stimulus discrimination, response differentiation, chaining, conditioned reinforcement, or punishment.

The first time I read anything Skinner himself had written was in Schoenfeld's class in Intermediate Experimental, which I took in the fall of 1943. We were assigned a passage, or perhaps two, that dealt with the contingencies of reinforcement under ratio and interval schedules (Skinner, 1938). Skinner's analysis of the relation between the animal's responding and the arrival of the reinforcing stimulus struck me as extremely shrewd, but schedules of food delivery in rat experiments did not seem to have much to do with my interests in clinical and social psychology.

Luckily, in the spring semester I took a job preparing and grading the examinations for the introductory course at Columbia College. That semester, it was taught by T. G. Andrews, who came over from Barnard, the undergraduate division for female students (on the other side of Broadway), to fill in for faculty who were away on wartime missions. I learned how to write relatively unambiguous test items, and I learned how desperate members of the ROTC could be for a passing grade. In the fall of 1944, the course was taught by Otto Klineberg, and I learned the importance of making sure I knew how to operate a movie projector before trying it in class. Finally, the next spring (1945) Fred Keller came back from Camp Crowder, where he had been developing a training procedure for the reception of Morse Code, and I learned the meaning of Skinner.

I still have the 20-page mimeographed

outline he handed out in that class: "The purpose of this course," it began, "will be to present some of the basic principles and problems of psychology. . . . Especial attention will be given to those principles possessing the greatest generality and having the widest practical usefulness. . . . The subject matter of psychology is the behavior of organisms as related to environmental and other 'variables.'" Eventually, Keller's outline went on to cover most of the basic concepts discussed in *The Behavior of Organisms* (Skinner, 1938), plus additional material on Thorndike's trial-and-error experiments, International Morse Code, latent learning, double alternation, the delayed reaction, Kohler's work on insight, the development of infant speech, Skinner's treatment of verbal behavior, the work of Ebbinghaus and of E. J. Gibson on the memorization of nonsense syllables, and the concept of "set." Constructing multiple-choice and true-false items is an excellent way to become familiar with a body of material, and I soon became impressed not only with the greater objectivity of Skinner's terminology but also with the idea that the processes Keller was talking about were indeed the very heart of psychology.

Another factor that contributed a great deal to my interest in behavioral principles was Keller's telling me about a manuscript Skinner had written, entitled *The Sun Is But a Morning Star*. I was strongly committed to the need for a new social order. During most of my years both at Dartmouth and at Columbia I had served on the National Executive Committee of the Young People's Socialist League, the youth section of the Socialist Party. In a brief period between undergraduate and graduate work I had even served as its National Secretary. During my student years I probably spent more of my time on political matters than I did on psychology. Consequently, when Fred mentioned a fictional utopia based on behavioral principles I was all ears, and a couple of years after the book finally appeared, under the revised title of *Walden Two* (Skinner, 1948b), I wrote a review for a radical youth publication named *Anvil and Student Partisan*. Unfortunately, my sympathetic review was at odds with the views the editors were hearing from their literary friends. My review made it into galley proof but never into print.

Newark. All along, my budget had been



Fig. 1. W. N. Schoenfeld (Courtesy Office of Public Information, Columbia University).

very tight. For lunch I bought a sandwich for 10 cents and a bottle of soda pop for the same price at a delicatessen on the other side of Amsterdam Avenue. My trip home on the subway cost only 5 cents, but I remember on many occasions debating whether I could afford another nickel for a newspaper to read on the way. We lived in a rent-controlled apartment at \$40 a month. With mounting debts and a new offspring to support, I desperately needed some income beyond that which my father was providing each month, and in the fall of 1945, at the age of 23, I took a job teaching 12 hours a week at a nearby institution then known as the University of Newark. (By the beginning of the following summer, the campus had become a branch of Rutgers.) I was paid \$2,400 for the academic year, or \$100 per credit hour. For the subsequent year, I was the entire faculty in psychology. As I recall, I taught two sections of introductory psychology each semester, along with personality and tests and

measurements. It is clear that I was greatly influenced by Keller's presentation: In the introductory course I handed out three legal-sized mimeographed sheets on which I described such basic behavioral processes as conditioning, extinction, spontaneous recovery, discrimination, and differentiation. I used Hilgard and Marquis' *Conditioning and Learning* (1940)—all there was at that time—to flesh out my lectures. I was excited by the material and thought I could transfer that excitement even to a group of part-time blue collar students, but that proved to be a more difficult task than I had anticipated.

My interest in Skinner was further heightened that same fall when his article on the baby tender appeared in the October issue of the *Ladies' Home Journal* (Skinner, 1945). My son, Daniel, was less than a year old at the time, and as our landlord was economizing on coal that winter we often had to keep the oven on all night to maintain his bedroom at what we considered an adequate temperature. Skinner's idea of building an independent heating and cooling system for the crib itself seemed eminently sensible, and I marveled at the versatility of a man who could both write a book like *The Behavior of Organisms* and come up with such a socially useful piece of engineering.

First rat. By that spring I had concluded that I wanted to conduct my dissertation in operant conditioning, but I had no notion of how to go about it. Fortunately, this was the year in which Keller and Schoenfeld instituted their introductory laboratory course at Columbia College, so the basic apparatus was now available, but I had never so much as touched a rat or a lever. With considerable diffidence, I confided my problem to Fred Keller. His reaction was characteristic: He set up an appointment for a Saturday morning, when the equipment was not in use, selected two untrained rats, and showed me how to remove the external food tray from their cages in order to restrict their food intake to a given hour of the day.

On Saturday, at the appointed time, we went to the vivarium and took out the rats, setting their cages on trays of sawdust to transport them to the classroom. There we placed the first tray and its contents on the table in the fourth or perhaps the fifth cubicle along the right hand side of the room. With

the Columbia apparatus, no shaping was ordinarily required to induce the rat to approach and press the lever; when the cross-bar was inserted into the living-experimental cage, the rat normally depressed it a number of times prior to any reinforcement (see Schoenfeld, Antonitis, & Bersh, 1950b). Explaining what he was doing at each step along the way, Keller delivered a small quantity of powdered food through the metal chute into a tray inside the animal's cage. After the rat had learned to approach the tray and eat the food, Keller inserted the bar and reinforced a number of presses. Then he left the room, telling me to train the second rat on my own. All went according to plan. I conditioned the rat's behavior, and the rat conditioned mine. Molding the behavior of another organism, without benefit of verbal communication, continues to fascinate me to this day. To me, it is the heart of psychology.

The School of General Studies

To teach at Newark, I had to commute three days a week via the Hudson Tubes and remain near the campus from before breakfast in the morning to fairly late in the evening. The university's library was minuscule, and even the public library did not contain many of the journals I needed. There were no laboratory facilities. On that schedule, I could prepare my lectures, and I was able to clear up my Incomplete in Advanced Experimental, but it would have been difficult to conduct a dissertation based on animal subjects.

Fortunately, about this time the veterans of World War II began to return in large numbers to the nation's campuses and experienced teaching personnel were suddenly in short supply. A position opened up at Columbia's School of General Studies, formerly known as the Extension Division. There, part-time and evening students could earn a fairly good undergraduate degree, bearing a Columbia label. The students were not always academically proficient, but they were usually relatively serious. During my first year at the School of General Studies, I was required to teach three sections of the first-semester introductory course each term and one of its second-semester sequel. This arrangement had the virtue of reducing the volume of material I had to prepare, but by

the time I gave the same lecture for a third time it was difficult to sustain my enthusiasm. The required text was Woodworth's (1944) *Psychology*, which contained a certain amount of material that was difficult to reconcile with a more behavioral approach, supplemented by selected chapters from Garrett's (1941) *Great Experiments in Psychology*.

During that year and the next, I tried out a couple of pilot procedures for a dissertation but was not happy with the results. I socialized with the people involved in the college course, especially Donald Bullock. Don was confined to a wheelchair, and I was impressed with the cheerful and apparently well-adjusted way in which he dealt with his disability. In the fall of 1947, Don left to take a teaching job at the University of Buffalo, but through letters and meetings he continued to be in close touch with the people at Columbia.

I had no further classroom contact with either Fred or Nat. Consequently, when I attended the first Conference on the Experimental Analysis of Behavior, held in Bloomington in June of 1947 (see Dinsmoor, 1987), I remained more an auditor than a participant. I was still learning, and Van Lloyd and I were somewhat in awe of more advanced students like David Anderson and Fred Frick, who played a more active role. I remember that I was impressed with the work of Bill Estes, who was at that time still working with rats and pigeons, and of course entranced by almost everything Skinner had to say. The 1947 and 1948 conferences were extremely important to my continued education in the experimental analysis of behavior. I was also fascinated by Skinner's material on verbal behavior, which he presented in a summer course at Columbia in 1947.

The Experimental Course

Another major contribution to my personal education came in the fall of 1947. David Anderson, who had been teaching the General Studies course in experimental psychology (GS 3), was called upon to teach one of the sections added to the Columbia College course, and I took over the assignment, teaching most of the sections until 1951, when I left for Indiana. GS 3 was a mixture of the old and the new. For the first few weeks we worked in an upstairs classroom with human subjects,

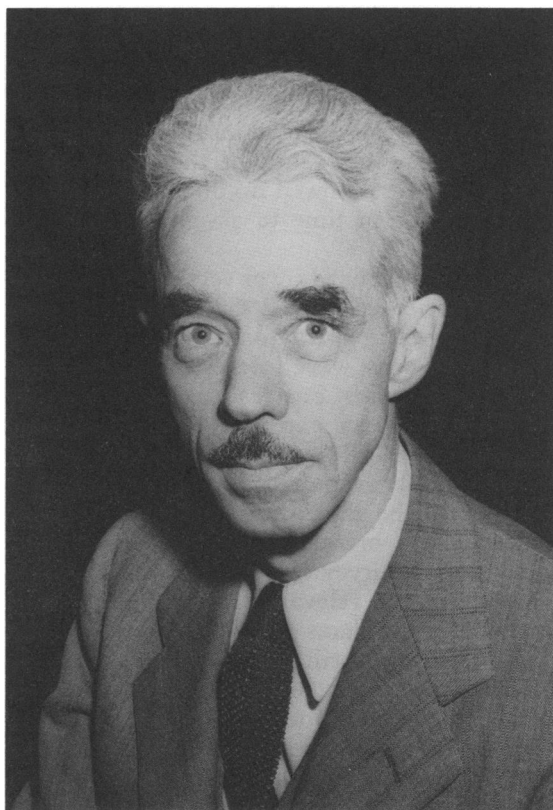


Fig. 2. Fred S. Keller (Courtesy Office of Public Information, Columbia University).

but later in the semester we went down to Room B13 in the basement, where a laboratory had been furnished with apparatus like that used in the College. This was one of the most enjoyable courses I ever taught. In the lectures I could concentrate on the material in which I was most interested, in the questions I handed out each week I could examine both methodological and theoretical issues, and in the experiments I could to some extent explore topics of genuine interest and significance. There were only a dozen students in each of the sections, and these often included one or two current or prospective graduate students who needed an experimental course to make up a deficiency in their preparation. In an earlier article (Dinsmoor, 1989), I listed Doug Anger (Western Michigan, Missouri), Alex Buchwald (Indiana), Aubrey Escoffery (Hampton University), George Kish (Maine), and Bob Thompson (Hunter, New York State Psychiatric Institute). To these I can now add Bob Berryman (Hunter, et al.) and Gus

Fink (Stony Brook). There were probably others whose contributions I have missed. The first few chapters of the mimeographed edition of Keller and Schoenfeld's book (King's Crown Press) began appearing in installments during the first semester I taught the course, and most of the rest showed up during the second semester, just in time to assign them to the class.

Although the hard cover version of the book was not released until 1950, even the mimeographed edition was of immense value to those of us who were interested in basic behavioral processes. For graduate students, the book provided an excellent survey of what was important, what was already known, and what was only suspected. It made it relatively easy to spot the gaps. It directed our research. It was also a morale builder, because as an introductory survey it provided authoritative evidence of the relevance of our research to the broader problems of the discipline. Believe me, it is a real thrill to see your dissertation summarized in a book designed for students taking their first course in psychology! It suggests that you have contributed to the very core of the subject matter.

The book also served as a stimulant for discussion. In the corridors, in the vivarium, and in our offices we refined our skills by arguing the merits of Skinner's approach versus that of Hull (1943), who was at that time the dominant figure in the experimental literature. (Kenneth Spence taught in one of the summer sessions while I was there, and his classes added to our sophistication on these issues.) In discussing our personal plans and activities, including our interactions with one another, we made frequent use of technical terms like discriminative stimulus, response, and reinforcement. The rehearsal increased our facility and strengthened our conviction concerning the utility of Skinner's conceptual system for dealing with the world around us. It seemed obvious that the new, more scientific psychology was vastly superior to the amorphous speculations of the past, and we looked upon ourselves as the vanguard of a revolution in the way people thought about psychology.

Ferster

To one degree or another, I counted a number of my fellow students as my friends, but there are two in particular that I would

like to single out, Charlie Ferster and Donald Cook. I first became acquainted with Charlie when he was appointed to assist me with the General Studies course in experimental psychology. One of our sections met on Monday evening, and we formed the habit of eating together before the class at a small Italian restaurant a couple of blocks down the hill on Amsterdam Avenue. Most of the time we ate pizza, which was at that time a special dish obtainable only at Italian restaurants. It was some years after I moved to Indiana before the first pizzeria opened in Bloomington and still more years before the national chains began to spring up. Over dinner, we often discussed the progress of the course and our plans for research. But it was also a social occasion. Sometimes we had a bottle of beer, and we got to know something of each other's interests and values. We were both democratic socialists. I remember that when the second Conference on the Experimental Analysis of Behavior came up in 1948, I rode to Bloomington with Charlie and his wife, Marilyn. Their car ran into some kind of operating difficulty, and we had to stop in Harrisburg for repairs, but we did make it in time for the opening of the conference. In 1957, when I was teaching in Bloomington and Charlie began working at the Indiana University Medical Center in Indianapolis, the Dinsmoors and the Fersters renewed the friendship.

Although it seems to me that he already thought quite independently, while he was at Columbia Charlie seemed less sure of himself than he did in later years, when his reputation had been established. He was quiet and self-effacing. There were people who thought he was a weak student, and as I recall, when he presented his first dissertation proposal to the departmental seminar, some of the faculty outside of our group expressed reservations. It is clear, however, that I sensed some of Charlie's potential. In 1949 we joined forces to plan and begin work on a study of conditioned reinforcement (Dinsmoor, 1952c), but Charlie had to abandon the project when he accepted a position supervising Skinner's laboratory at Harvard. In late 1950 or early 1951 I visited Charlie in Cambridge and copied down a few tricks of circuitry, some guidelines for working with pigeons, and the basic specifications for the electro-mechanical programming modules he had designed. Shortly after my return, I acquired

three pigeons—the first at Columbia—but encountered some technical difficulties and failed to collect any data before leaving for Indiana. About a year later I began constructing my own programming modules based on Charlie's specifications, using some military surplus relays I had purchased with personal funds down on Canal St.

Fred Keller also had faith in Charlie and served as the sponsor of his dissertation. Charlie's first project, as I recall, was a study of how a new unit of behavior might be formed by combining two bar presses. Fred had already conducted some pilot work, using graduate students as subjects and escape from light—based on verbal instruction—as the source of reinforcement (Keller, 1977, p. 21). Charlie did it with rats. Keller reported some preliminary findings at the first Conference on the Experimental Analysis of Behavior (see Dinsmoor, 1987). According to the notes that I took at the time, spontaneous "doubles" appeared rather suddenly, in the form of presses that came less than half a second apart. If such pairs were reinforced, they could often be maintained as a separate class of behavior while single presses were being extinguished. I'm not sure why Charlie abandoned this work, but I suspect that he needed something that was less of a gamble, that could be completed quickly and reliably prior to leaving Columbia for Harvard. He referred to the PhD as his "union card" for working in the academic profession. Nevertheless, the original idea seems to have been a good one. Subsequently, Justin Carey made use of single and double presses as his prototype responses in a dissertation demonstrating regression to an earlier mode of behavior when reinforcement was withheld from a more recently acquired class of responses (Carey, 1951).

In the meantime, Charlie completed a dissertation (Ferster, 1951) in which he found that bar pressing trained in a lighted chamber manifested equal strength whether extinguished in the presence of that same stimulus or in the dark. He was not happy with this result, however, as it appeared to be in conflict with findings obtained by other investigators (Cook, 1950; Skinner, 1950).

Cook

My friendship with Donald Cook developed toward the end of my stay at Columbia but has persisted over the ensuing years. It was

based not only on the similarity of our professional interests but also on an enjoyment of some rare and sometimes underappreciated talents. Beginning with shop talk, I soon discovered that Don was this century's equivalent to a renaissance man. Earlier, he had planned to become an engineer but had switched to psychology as a major. He served on the staff of the *Columbia Review*, along with Allen Ginsburg. He had taken the introductory laboratory at Columbia College and gone on to become a graduate student in psychology. He had a wide-ranging curiosity, an enormous knowledge of literature and of music, a willingness to entertain the most unconventional of ideas, keen analytic ability, and an uncommon appreciation for and skill with the subtleties of the English language. At the time I left New York and for a few years thereafter, his apartment on 112th St. served in the evenings as a sort of intellectual and artistic salon, a gathering place where young psychologists, jazz musicians, Reichians, early computer scientists, composers, and some destined to become among the best known literary names of that generation mingled and interacted (for a thinly disguised but fictional account, see Johnson, 1983, pp. 57 ff.) Here, psychology took on all comers.

Apparatus

In those days, people did not purchase their equipment ready-made from commercial vendors. In the first place, there was no money, other than the modicum provided by the departmental budget. In the second place, there were no vendors. Apart from a few items that we could have purchased from the departmental "shop man" at Indiana, there were no commercially available levers, feeders, or experimental chambers, let alone cumulative recorders or programming modules. But at Columbia we were fortunate in having a substantial metal and wood-working shop, staffed by a full-time machinist named Fred Blendinger. There we learned some elementary construction skills—how to operate drill presses and various types of power saw, thread rods and holes, countersink wood screws, mill metal, strip and solder wires, and so on—that might one day be needed to construct our own laboratory facilities. I found that the opportunity to work with material objects, as well as with words and numbers, greatly

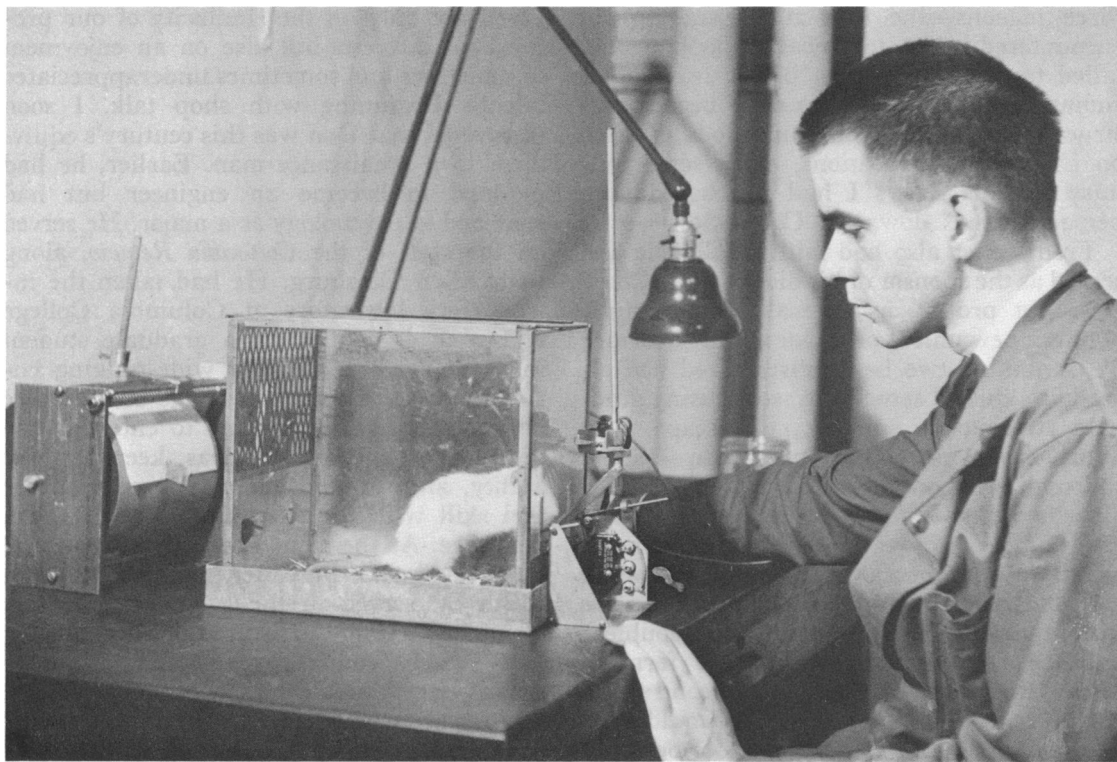


Fig. 3. The author training a rat in a demonstration chamber, circa 1950. With the exception of the clear plastic walls of the chamber, the apparatus was standard for the period. (Photo by H. Leibowitz)

enriched my existence, lending greater variety to my professional duties than I might have enjoyed in almost any other profession.

One of the many side benefits of the Columbia College course was that it led to the design of a standard set of apparatus and the construction of a sufficient number of copies to equip each of the cubicles. Then, once the needs of the classroom had been met, additional units were turned out for research purposes. The operant chamber was extremely versatile. Whatever experiments we students might dream up could be accommodated, provided we could use the albino rat as our experimental subject, bar pressing as our index response, and powdered lab chow—later, pellets—as our reinforcer. (Aversive control was and still is more difficult.) There was not a large literature to check. In most cases, it did not require much time or effort to proceed from the formulation of an experimental question to setting up an empirical test in the laboratory. As I recall, there was usually a spare rat or two on hand

for pilot work, and departmental funds were available to purchase larger groups, once we were committed to a more formal design. In short, it was an environment that made it exceptionally easy for anyone who was so inclined to begin conducting experiments.

Most of us used extra, or in the off-season, borrowed copies of the living-experimental cages built for the College. Each rat currently serving as a subject in someone's experiment was housed in its own, numbered cage, either in the vivarium or directly in the laboratory. When the time came to start an experimental session, we moved the cage into position and inserted a lever through the H-shaped slot cut in one end.

The use of the animal's normal living quarters as an experimental chamber made the initial conditioning of bar pressing relatively easy. It was not necessary for the subject to adapt to a new environment. Also, as the bar was mounted on its own platform, it could be raised or lowered with respect to the floor of the cage. Lowering the bar presumably

increased the unconditioned rate and made pressing easier to condition. First we dropped a few pellets of food, one at a time, into the cage to train the animal to seize and eat them whenever it heard the sound of one hitting the tray. Then the crossbar and its supporting shafts were inserted. Usually the rat came over within a very short time, depressed the bar, and received and consumed its first reinforcer. From that point on, the rest was relatively easy.

During the early years, most of the work at Columbia was conducted by means of hand-switched lights and hand-delivered pellets in darkened rooms, with only dim red lights to make it possible to write down the data. It was assumed that the slightest sound would disturb the rat's performance, and people carefully tiptoed in and out of doors bearing signs that announced "Experiment in Progress." Such precautions did not always avail: There was nothing Al Libby could do, for example, when a parade, replete with marching bands, tramped past his window during a crucial test session that happened to fall on St. Patrick's Day. At the Conferences on the Experimental Analysis of Behavior we had seen picnic ice boxes used to isolate the individual subjects from the rest of the laboratory, but such enclosures required automated control and recording apparatus, and that was slow in coming.

The shift began about the time I started my dissertation. My first insulating shell, built specially for that occasion, was a plywood enclosure with a Celotex® lining and an access door through which I could reach in and deliver a pellet. Then in 1949 I was fortunate enough to inherit as my laboratory and office a room that included a closet-sized sound-resistant chamber (see Frick, 1948; Hefferline, 1950; Keller, 1977, pp. 18 ff.).

Although items like the levers, cages, pellet dispensers, event recorders, and kymographs used in the undergraduate course were readily available, automated programming circuits were another matter. Few of us had much idea of how these might work. There were books written for ham radio operators and for budding electrical engineers, but none that addressed the problems with which we were concerned. Years later I reached a level of expertise at which I could lecture for a dozen hours or more to the graduate students at

Indiana, but the necessary information was accumulated bit by bit from a great variety of sources. It was a painfully slow process.

My first piece of programming apparatus was a flat strip of aluminum about 3 feet long by $\frac{3}{4}$ inch wide, mounted on the shaft of a constant-speed (clock) motor. The ends were cut on the diagonal, so that each time they passed they depressed the arm of a pressure-activated microswitch mounted at the base of the apparatus. My reasoning was that the length of the aluminum strip should amplify its lateral motion and enable me to adjust with relative precision the length of time for which it depressed the arm of the switch. Unfortunately, the reverse leverage also reduced the torque, with the result that the arm tended to stall whenever it contacted the switch.

In 1948, Donald Cook brought back from the second Conference on the Experimental Analysis of Behavior a more sophisticated device, which had been donated by Skinner. Instead of a long arm the motor turned a 16-inch transcription disk, like those used at the time in the control booths of broadcasting stations. The surface of the disk was non-conductive, but small slits were cut in it at carefully measured intervals around the circumference to reveal the metal underneath. A metal stylus rode on the surface, and whenever it touched the underlying core of the disk through one of these slits the electric circuit set up a reinforcer to be delivered following the next response. What was especially useful was that the disk permitted the use of a variable interval or, as we then called it, "aperiodic" schedule of reinforcement.

I also have a visual image of the apparatus I used to keep track of time during the training phase of my dissertation research (Dinsmoor, 1950). My procedure called for the introduction of the positive discriminative stimulus (the lighting or darkening of the experimental chamber, counterbalanced across animals) only after the rat had gone for 30 seconds in the presence of the negative stimulus without pressing the lever. I could have timed this with a stopwatch, I suppose, but it was more convenient and probably more accurate to use a device specially built for the occasion. This was Fred Blendinger's idea. A brass collar was fitted to the shaft of the timing

motor with a tolerance carefully adjusted so that the collar normally turned with the rotation of the shaft but could be pulled back when necessary to another position. A hole was drilled in this collar and threaded to accept a section of $\frac{1}{8}$ -inch diameter brass rod, which served as the timing arm. The timing arm was connected to a metal plunger that fit into the hollow core of an electromagnetic coil (solenoid). As the arm moved toward its destination, it gradually pulled the plunger farther and farther out of the coil, but each time the rat depressed the lever the electromagnetic action of the coil pulled the plunger, and with it the timing arm, back to its original position. (Watching this device for 10 days at a stretch, I became very aware of the sexual symbolism, as the plunger repeatedly withdrew at a very slow pace, only to slam itself forcefully back into the empty tube.) When the timing arm finally reached a machine screw threaded into the mounting plate, I turned the light in the experimental chamber on or off, as the case might be, waited for the rat to depress the lever, and then opened the door and dropped a pellet of food into the chute leading to the tray inside the animal's cage.

My next experiment was fully automated. I nailed some 110-volt AC relays onto a section of plywood and "hard" wired them to each other with soldered connections. A cam mounted on a timing motor operated a pressure-actuated switch once each minute. Depending on the status of an old-fashioned brass and ceramic knife switch, a light in the chamber might be turned on or off or remain unaffected (for different groups) at the end of the second minute. If the change did not occur at the end of the second minute, it came at the end of the third. Also, a pellet of food was "set up" at the end of the third minute and delivered following the next response (Dinsmoor, 1951). Automated circuits were unlike anything I had previously encountered; even though of my own creation, they seemed almost magical; they gave me a strange feeling of mastery over the physical world. Nevertheless, I found it ironic that visitors to the laboratory seemed more impressed with my instrumentation than with the research itself.

I think it must have been Paul Wilson who was responsible for the next step forward,

the construction of a more versatile assemblage in which the connections between the relays could be altered to suit a variety of experimental procedures. In any case, before I left Columbia I recall working with three sets of 110-volt AC relays mounted in a fiberboard cabinet. The coils and the switching contacts were connected to jacks on the surface of the cabinet, and these in turn were linked together by means of wire leads (electrical cords) with small plugs at either end, much like a telephone switchboard. (Aptly enough, Wilson went on to work at Bell Laboratories.) Now the programming and recording circuits could quickly be modified; sometimes we used the same set of relays to control two or more different experiments within the same day. However, I did not become acquainted with the Bakelite clip-on panels that would subsequently become the hallmark of operant laboratories throughout the country until my visit with Charlie Ferster at Harvard.

RESEARCH TOPICS

At Columbia we were certainly aware that behavior was complexly determined. Many of us were interested in clinical and social psychology. But our research was based on the hope that in psychology, as in other sciences, it would prove useful to analyze the more complex, naturally occurring patterns of behavior as interactive products of relatively simple, perhaps elementary processes, that could be studied one by one in the laboratory. We believed that much of the activity examined by other psychologists—including many of those working with rats and monkeys—could be broken down into combinations of such constituent processes as reinforcement, extinction, stimulus generalization and discrimination, response induction and differentiation, chaining, and so on. These processes were the key to the integration not only of different levels of psychology but also of different areas of application. To provide a systematic account of our subject matter, then, it was necessary to understand the principles of conditioning and how these principles interacted with each other to produce more complex forms of behavior. In this enterprise, we were privileged to be in on the ground floor. Although Skinner had provided a methodology by means of which these underlying processes could be studied cleanly and rig-

orously, information on even their most rudimentary characteristics remained extremely fragmentary. There was an atmosphere of excitement. Very little lay behind us and a great deal lay in front of us. We were on to something big. We felt like pioneers on the edge of vast, uncharted territories, reputed to hold enormous riches.

Aversive Control

To a considerable extent, the research conducted during the early years of the Columbia program reflected the interests of our teachers. For example, Fred Keller had extended Skinner's basic conditioning paradigm from positive reinforcement, using pellets of food, to negative (aversive), using the termination of the light from a 25-W bulb as his reinforcing agent (Keller, 1941; reprinted in Catania, 1968, pp. 188–194). In collaboration with K. W. Oberlin (Keller & Oberlin, 1942), he had also devised an apparatus for measuring the degree of preference the rat exhibited between light and dark. This apparatus contained two chambers, separated by a 2-inch hurdle, which rested on a tilting floor, so that a continuous record could be kept as to which compartment the rat currently occupied. The rat's location determined whether the light was on or off and the relative time spent in each compartment served as the dependent variable. Later, Linc Hanson (1951) used a similar device, varying the intensity of the light and noting the change in the percentage of time the rat spent in the darkened compartment.

The earliest of the dissertations using light, however, was one by Ralph Hefferline (1950), who taught the College course in abnormal psychology. Hefferline carried out a very careful set of experiments, culminating in his famous "holding" procedure: When the rat depressed the crossbar, the light went off, but as soon as the animal let the bar up, the light came on again. The result was that his subjects held the bar down for long periods of time, often amounting to as much as 97% of the experimental session. Hefferline suggested that because the proprioceptive stimulation from incipient releasing movements was intermittently followed by (thus, paired with) the return of the light, this stimulation might also become aversive; that is, its termination might become reinforcing, leading the rat to renew its pressure.

Under Keller's sponsorship, Martin Tolcott (1948) extended Hefferline's work to a conflict situation in which standing on a pedal or platform at one end of the chamber kept the light off but depressing a lever at the other end produced pellets of food. In a third study, Wilma Winnick (1956) succeeded in recording examples of the incipient releasing movements postulated by Hefferline, using as her response device a small hinged wall panel with a stylus attached. The stylus left a continuous tracing on moving tape of how far the rat was pushing the panel in at any given moment during the experimental session. Gradual releases, which almost allowed the light to come on, were followed by sharp restorations of pressure.

Perhaps as a consequence of his dissertation, Hefferline developed a strong interest in the general topic of feedback from the subject's own behavior, especially the stimulation arising from conflicting responses that blocked other behavior from free expression. In his course in abnormal psychology, he developed a series of exercises designed to increase the student's awareness of (ability to discriminate) such stimuli (see Hefferline, 1958, 1962). This exercise became an important component of a treatment program known as Gestalt therapy (Perls, Hefferline, & Goodman, 1951).

When all occurrences of the target response were reinforced by its termination, bright light was effective as a laboratory stimulus. But when attempts were made to extend the technique to intermittent schedules, matters became more difficult. When the light remained on for extended periods, the rats apparently learned other ways of alleviating the situation, such as covering their eyes or hiding their heads in shadowed areas, which conflicted with the response chosen by the experimenter. It was Mac Parsons, initially, and later Mike Kaplan, who worked on a technique for dealing with the problem. First the rat was trained to stand for long periods of time on two rods or pegs jutting out from a smooth vertical surface. Once it had mastered this trick, it was presented with the light, which was now unavoidable and inescapable except by depressing the lever (for a description, see Kaplan, 1952). Even so, Kaplan found that beyond a certain level of illumination the rate of responding began to decline, rather than to increase, with further increases in the intensity of the stimulus. Mindful of the difficulties these inves-

First Seminar Report - Murray Sidman - October 24, 1950

Some preliminary data have been obtained using a new procedure for avoidance conditioning: the animal receives a brief intense shock whenever the bar is not pressed for x seconds. No exteroceptive stimulus is manipulated by the experimenter. Bar-pressing does not terminate the shock, but only delays its occurrence. The only variable manipulated by E is the minimum time after which a shock can follow a bar-press. Results: the animal develops and maintains a high rate of bar-pressing. It is intended to confirm this finding with more animals and with stricter control of certain possible variables.

Exp. I is designed to secure information on two variables assumed to be interacting under the present procedure. According to one theoretical account, bar-pressing gains in strength (in avoidance conditioning) at the expense of non-bar pressing behavior which is depressed by shock. Presumably the extent to which non-bar pressing behavior is depressed will be a function of the frequency of occurrence of the shock. Thus, as the length of the inter-shock interval is decreased from some high value, non-bar pressing behavior will decrease in rate (because it receives shock more often) and the bar-pressing rate will increase. This function may be called the "distribution of punishment" gradient.

As the inter-shock interval decreases, however, there is a simultaneous decrease in the minimum time after which bar-pressing will be followed by shock. At short intervals, therefore, bar-pressing will begin to be "hit" by the shock and its rate will now decrease as the shock interval decreases. This can be called the "delay of punishment" gradient. The interval at which these two gradients begin to interact should show a maximum in the rate = $f(\text{shock interval})$ curve. Some preliminary data have been obtained on this point.

Exp. II is designed to eliminate the influence of the "delay of punishment" gradient while manipulating the "distribution of punishment" gradient. If this eliminates the maximum in the rate = $f(\text{shock interval})$ curve, the present analysis will be supported.

Fig. 4. Mimeographed sheet passed out by Murray Sidman when he presented his dissertation proposal to the departmental seminar at Columbia. (By permission)

tigators had encountered, when I began studying escape conditioning a few years later I decided to use a mild level of shock, rather than bright light, as the stimulus to be terminated (for a review, see Dinsmoor, 1968).

Of all the work coming out of the Columbia laboratories while I was there, the best known and most influential was Murray Sidman's dissertation (Sidman, 1953a, 1953b). He, too, used electric shock. Animal rights activists often write as if the only motivation scientists could have for using shock is a sadistic desire to see their subjects suffer. This certainly was not true in Murray's case. In more recent years he has combined his research knowledge with more anecdotal material in a highly detailed and strongly worded critique of the use of aversive stimuli to control behavior at the applied level (Sidman, 1989b).

By the time Sidman began his thesis, Schoenfeld had developed a classic analysis of avoidance that continues to influence my thinking and that of other people long after the author himself has abandoned it. The basic

theme of Schoenfeld's analysis was that all stimuli other than the warning signal and all responses other than the one selected by the experimenter as the response to be learned by the subject were at some time or other followed by the shock. Only the target response in the presence of the warning signal was free of subsequent shock (Schoenfeld, 1950), and the change from behavior that was paired with shock to behavior that was not paired with shock was reinforcing. Extending this analysis, Sidman reasoned that it should be possible to do away with the warning signal and to train an arbitrary response, such as pressing a lever, simply by arranging the contingency that it never be followed (within a specified number of seconds) by the shock. In his dissertation, he first demonstrated that the procedure did condition lever pressing by the rat (Sidman, 1953a) and then went on to examine the effects of varying two parameters—the time between the response and the shock (R-S interval) and the time between successive shocks when no response occurred (S-S interval) (Sidman, 1953b; reprinted in Catania, 1968, pp. 196–203)—on the rate at which this response occurred. (For a reminiscence about this work, see Sidman, 1989a.)

Sidman's unsignaled or "free operant" avoidance procedure was widely used in subsequent research, both by Sidman himself (for a review, see Sidman, 1966) and by other investigators. His dissertation was cited in many textbooks, as well as in the research literature. And the overall experimental design had a major impact on the way research was subsequently conducted in operant laboratories.

Discriminative and Reinforcing Functions

One of Nat Schoenfeld's major interests was in secondary, as we then called it, or conditioned reinforcement. The first of the Columbia studies on the topic was conducted by Schoenfeld himself, with the assistance of Joe Antonitis and Phil Bersh (Schoenfeld, Antonitis, & Bersh, 1950a). After obtaining negative results with a stimulus that was regularly presented while the animal was consuming the primary reinforcer (i.e., after the onset of eating), the authors suggested that it might be necessary (as well as sufficient) for the stimulus to become a discriminative stimulus for some response before it could serve effectively

as a reinforcer. This was known as the discriminative stimulus hypothesis of conditioned reinforcement (see also Keller & Schoenfeld, 1950).

Several dissertations followed. In a straightforward parametric study, Phil Bersh (1951) explored two of the most important variables determining the potency of a stimulus as a conditioned reinforcer: (a) the number of pairings and (b) the time between the onset of the stimulus and the delivery of the reinforcer. Using very different techniques, Joe Notterman (1951) and I both examined the relationship between the discriminative and the reinforcing functions of the stimulus. Joe used a runway. With a constant number of S+ trials, he found that the more S- trials he gave the subject the more effective the positive stimulus became when used as a reinforcer during a subsequent extinction test. I trained my rats to discriminate between a lighted and a darkened chamber and then used the positive stimulus either discriminatively (preceding the response) or as a reinforcer (following the response) during a series of test sessions. Both groups yielded approximately the same extinction curves and in both cases the number of bar presses was substantially higher than for a group that did not receive the stimulus (Dinsmoor, 1950; reprinted in Kimble, 1967, pp. 408-427).

My next study of conditioned reinforcement began as a joint effort with Charlie Ferster, but the collaboration ended when Charlie moved to Harvard to oversee the Pigeon Project (see Ferster, 1970). A finding that was considered very important in those days, confirmed in a number of published studies, was that intermittent reinforcement of a response increased its subsequent resistance to extinction. I extended the question to ask whether a conditioned reinforcer could similarly be made more resistant to extinction by changing the schedule of primary reinforcement in its presence. That is, would turning the stimulus on well before the food pellet became available, so that the schedule in its presence became fixed-interval rather than continuous reinforcement (i.e., fixed ratio of one), produce a longer lasting conditioned reinforcer? I discovered that it did (Dinsmoor, 1952c; for another scheduling parameter, see Dinsmoor, Kish, & Keller, 1953). The same conclusion has subsequently been reached by a variety of

other investigators, several of whom appear to have raised the question quite independently.

But in the meantime I also discovered that I had an improved technique for studying the formation of a discrimination. As the positive stimulus was present for an extended period of time and the animal did not eat after each response (cf. Frick, 1948), I could obtain a meaningful rate of responding in the presence of that stimulus as well as in the presence of the negative stimulus. The use of the same measure for both stimuli made it possible to compare the two performances and to calculate a single ratio or percentage that indicated how well the subject was discriminating between the two stimuli (Dinsmoor, 1951). It was obvious that a variable-interval schedule would yield a more stable rate of responding, and in my next study (Dinsmoor, 1952b; reprinted in Verhave, 1966, pp. 389-398), I borrowed Donald Cook's disk, described above, for generating such a schedule. I also took a look at the effect of changing my rats' level of hunger, using several different body weight criteria to regulate their food intake. I found that the relationship between the two rates of responding remained invariant despite large changes in their absolute level: The discrimination itself, as measured by the proportion of responding that occurred in the presence of the positive stimulus, was not affected.

I continued to be interested in clinical and social psychology. It seemed to me that my study of the effect of the hunger drive on the discriminative performance of the rat threw some light on previously published studies of drive and "perception" with human subjects, particularly those directed specifically toward the hunger drive and the perception of food-related objects. Accordingly, I included a review of those studies in my introduction and boldly published my data in the *Journal of Abnormal and Social Psychology* (Dinsmoor, 1952b). I sent a report on the retention of this type of discrimination over an extended period between the training and test sessions to *Science* (Dinsmoor, 1952d). I also conducted some work in which I studied the use of differential punishment rather than differential reinforcement to establish the discrimination, and to reach out to a different audience I published the findings in a British journal (Dinsmoor, 1952a). This was the beginning of my interest in the avoidance theory of punishment. Some-

where around the same time I became aware that Donald Cook was also interested in discrimination training (see Cook, 1950), and to demonstrate some of its ramifications we collaborated on a project in which we first established a discrimination by the use of differential reinforcement and then reversed it by adding differential punishment. This, it seemed to us, provided a laboratory model for the study of a process the Freudians called "displacement."¹

The technique of using variable-interval schedules, rather than continuous reinforcement, to study the development of a discrimination was subsequently adopted by Bill Cumming for a dissertation (Cumming, 1955) in which he replicated earlier work by Frick (1948) and by Raben (1949). Using pigeons this time, Cumming examined the effect of the magnitude of the physical difference between the positive and the negative stimulus, coupling it with several different frequencies of reinforcement. The variable-interval schedule soon became the standard technique for studying the formation of a discrimination.

Operant-Respondent Interactions

Our main interest was in operant conditioning, as this form of behavior had a direct and visible impact on the physical and social environment. But we did not entirely neglect Pavlovian procedures, as these seemed to apply to underlying emotional states that produced important changes in the operant behavior (see Keller & Schoenfeld, 1950). Al Libby (1951), for example, determined the degree to which bar pressing was suppressed as a function of two of the parameters of the Estes-Skinner "anxiety" (conditioned suppression) procedure, the number of pairings and the time between the onset of the light (CS) and the presentation of the shock (US). Al was hired to teach clinical psychology at Indiana the year before I came there but left, demanding a higher salary, the year after. He was very supportive during the year that we overlapped. (Maressa Hecht Orzack and her husband, a sociologist, were also in Bloomington for a time.)

During the last 20 years or so, research on

conditioned suppression and on autoshaping, both of which are conventionally classified as forms of Pavlovian conditioning, has enjoyed such a surge in popularity that it is difficult nowadays to realize how little was known prior to, say, 1968. The first systematic work on the conditioning of changes in heart rate, another possible index to "anxiety," was carried out in a series of studies by Joe Notterman and Phil Bersh, working on postdoctoral fellowships with Nat Schoenfeld. The subjects were male undergraduates from Columbia College. The conditional stimulus was a 1-s tone, which was followed 6 s later by a 6-s shock through the left hand (e.g., Notterman, Schoenfeld, & Bersh, 1952a). One of the most surprising findings (Notterman, Schoenfeld, & Bersh, 1952b) was that in this preparation the conditional response was opposite in direction to that originally elicited by the unconditional stimulus. Although the cardiac response to the shock itself was an acceleration, the response to the tone was a decrease in rate. This finding has posed a substantial difficulty for the traditional description of Pavlovian conditioning as merely the substitution of one stimulus for another as an elicitor of the same response (see Pavlov, 1927). In later work, Bersh, Notterman, and Schoenfeld (1956) found that the acquisition by the subjects of a skeletal response that prevented the occurrence of the shock (i.e., an avoidance response) led to a significant decrease in the magnitude of the conditional heart-rate response, apparently because of the negative correlation between either a proprioceptive or an exteroceptive feedback stimulus (safety signal) and the shock.

And Other Problems

Others among us struck out in other directions. For example, Joe Antonitis (1951) built an apparatus with a long horizontal slot into which the rat could thrust its nose and a photocell beam that was interrupted whenever it did so. By photographing the position of the animal's nose along the slot at the moment when the beam was interrupted, Joe could calculate and plot the variability in this dimension during the conditioning, extinction, and subsequent reconditioning of the response.

Ray Strassburger (1950) examined the effect of the "hunger drive" (hours of food deprivation) at the time of conditioning on the number of bar presses during the subsequent

¹ Cook, D. A., & Dinsmoor, J. A. (1954, April). *A laboratory model for the study of displacement*. Paper presented at the meeting of the Eastern Psychological Association, New York City.

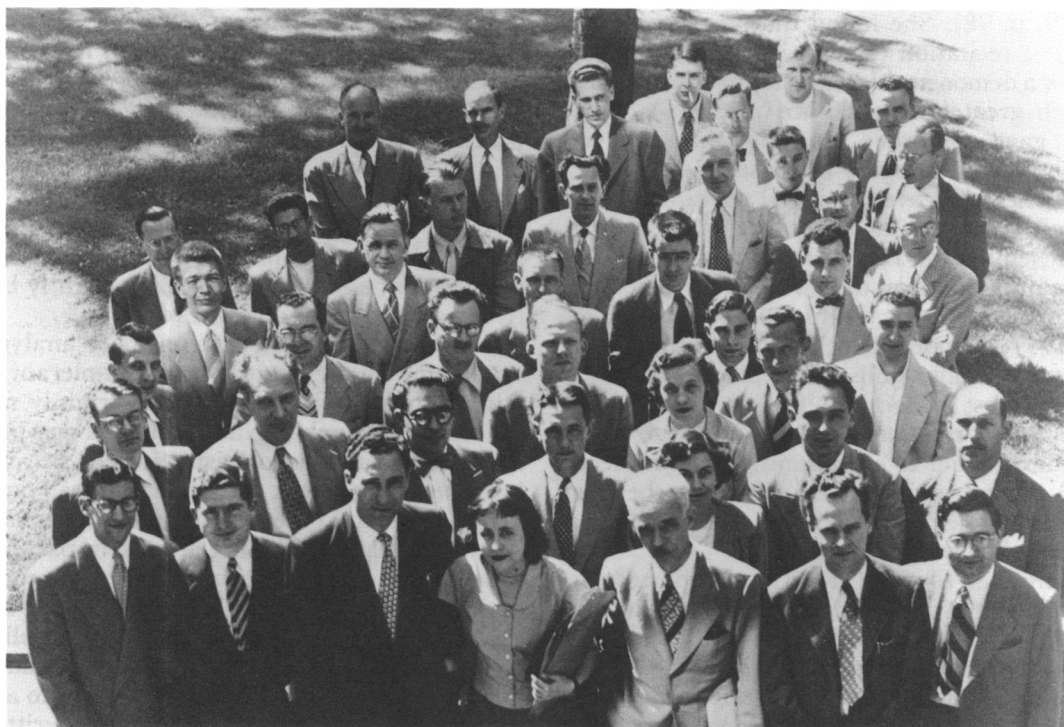


Fig. 5. Group photograph of people attending the third Conference on the Experimental Analysis of Behavior (1949). Taken from the second floor of Schermerhorn Extension. The participants from Harvard, Indiana, and North Carolina are indicated by the letters H, IU, and NC. All the rest were associated with the Columbia department. Left to right, first row: Mike Kaplan, Donald Perlman, Nat Schoenfeld, Ruth (Morris) Bolman, Fred Keller, Fred Skinner (H), Phil Bersh. Second row: Harold Coppock (IU), Ralph Hefferline, Helmut Adler, Fred Frick, Elaine (Hammer) Graham, Joe Notterman, Bill Jenkins (IU). Third row: Ben Wyckoff (IU), Joel Greenspoon (IU), Bill Daniels (NC), Van Lloyd, Dorothy Yates, unknown, Norm Guttman (IU). Fourth row: Lloyd Homme (IU), Joe Antonitis, Sam Campbell (IU), Jim Dinsmoor, Charlie Ferster, George Collier (IU). Fifth row: unknown, Burt Wolin (IU), Doug Ellson (IU), Fred Lit, Clancy Graham, Bill Verplanck (IU), Bill Estes (IU). Sixth row: Mac Parsons, Dave Anderson, Don Page, Murray Sidman, Phil Ratoosh, George Roth. Seventh row: Don Cook, Rod Funston.

extinction, concluding that there was none. All that was required was that the rat consume the pellet. In a 1951 dissertation, Charlie Crocetti (1962) used a more complex design. First, he trained his rats at a single, intermediate level of drive (7 hours of deprivation) and then extinguished the response at several different levels. The number of responses during extinction was a linear function of the number during earlier operant level determinations (i.e., prior to conditioning) at corresponding levels of hunger but did not seem to be affected by the degree of hunger at the time of conditioning. I cannot now recapture why it was that Charlie's rats had to be trained 12 at a time, but I remember that a dozen of us gathered in his lab one evening to train six successive squads of rats. Only two of the 72 animals failed to acquire the response within the allotted hour.

Nowhere else in the world, I suspect, could 12 experimenters have been assembled who could have been that successful in training Charlie's rats!

After taking Skinner's course on verbal behavior in the summer of 1947, Anne Ritter conducted a dissertation in which she briefly trained one group of subjects to respond to each of a series of nonsense syllables with the same syllable (echoic response), another group to respond with a different syllable from the same list (nonechoic response), and a third group to respond echoically to a consistent sequence of syllables (chain). On the test, she presented the same syllables again, mixed with other syllables, and instructed her subjects: "Whenever any one of the original four syllables is presented, then I'd like you to say whichever one of those four you think of first" (Ritter,

1949, p. 98). She found that "with only one pretest recitation (E1, P1, F1) all subgroups show a demonstrable tendency to echo at a level much greater than chance" (p. 116). When she gave continued training on echoic responses, that category increased in frequency, but when she gave continued training on non-echoic responses the latter category increased.

Schedules of reinforcement were not the consuming passion they were later to become, but some work was conducted along those lines. In 1947, Donald Bullock collected but for some reason did not publish (except in the Conference Notes; Bullock, 1949) data showing substantial correlations between the rate of responding prior to conditioning (operant level) and during extinction and likewise between the rate of responding during reinforcement at a fixed interval and extinction.

Although Skinner (1938) had reported very briefly on the matter (pp. 306–307), Paul Wilson and Fred Keller (Wilson & Keller, 1953; reprinted in Catania, 1968, pp. 79–82) were the first to publish any systematic, quantitative data on the development of spaced responding under the schedule that later came to be known as "differential reinforcement of low rates" (Ferster & Skinner, 1957). Whenever the rat went for 30 seconds, say, without pressing the lever, the next press became eligible for reinforcement. Wilson and Keller's article led to a series of studies in which various investigators attempted to evaluate the role of collateral behavior in the timing of the reinforced press under such a regimen.

In his dissertation research, Wilson (1954) examined the effect of the length of time between reinforcers on the rat's performance under a fixed-interval schedule. Over a range from 10 seconds to 6 minutes, the number of presses was a positively sloped but negatively accelerated function of the duration of the interval. He noted that the form of the function was the same as that previously reported by Kaplan (1952) over a similar range of intervals for pressing reinforced by turning off an overhead lamp. However, the function relating the number of presses in extinction to the length of the interval passed through a maximum at approximately 1 minute and thereafter declined. Wilson also found that the number of responses in extinction was approximately a linear function of the number of reinforcers that had been received.

Using essentially the same apparatus, John Boren (1961) varied the number of responses required for reinforcement on a fixed-ratio schedule. He found that the rate of pressing was a positive but negatively accelerated function of the size of the ratio from 2 to 21. The length of the initial run during extinction was virtually a linear function of the size of the ratio, but the total number of responses reached a maximum and began to decline slightly between a ratio of 15 and one of 21.

I have heard it said that behavior analysts are narrow in their interests and intolerant of other points of view. This was certainly not the case at Columbia. Nat Schoenfeld was primarily a social psychologist before he became interested in the work of Skinner and had published in several other areas. Fred Keller had been brought to Columbia at least in part because of his expertise in presenting the competing schools of thought in psychology. At the end of each of the chapters in their book (Keller & Schoenfeld, 1950), the authors appended notes that alerted the interested student to additional readings, most of which were written from other points of view. When David Zeaman proposed a dissertation in which he would test a prediction from Hull's (1943) *Principles of Behavior*, using a runway rather than the usual bar-pressing apparatus, Fred and Nat agreed to sponsor it (Zeaman, 1949). As predicted, the rat's starting time (latency) and running time were both inverse functions of the magnitude of the reinforcer—different sized cubes of processed cheese. Changes in the size of the reinforcer after the response was learned produced contrast effects, the first type of contrast to be noted in the animal-learning literature. Because his theoretical orientation was different, the name Zeaman may not be familiar to readers of this journal, but at the time of his death in 1984 Dave was the editor of *The Psychological Bulletin*.

EXPERIMENTAL DESIGNS

The experimental designs used at Columbia while I was there were quite different from those used in later years for the experimental analysis of behavior. For one thing, when we were dealing with processes like escape training, respondent conditioning, or the formation of a discrimination, the object of interest was often the initial *acquisition* of a given pattern of behavior, rather than the subsequent levels

of performance. Even Murray Sidman, who became the leading spokesperson for steady-state methodology (see Sidman, 1960), published data on his subjects' acquisition of un-signaled avoidance responding (Sidman, 1953a). Note that this interest in the way in which the behavior was originally established was entirely consistent with most of Skinner's work up to that point (see Skinner, 1938, 1948a, 1950) and with his later work on programmed instruction (e.g., Skinner, 1954). The first substantial body of research in which Skinner compared the results of a succession of procedures applied to the same individual was in his work with Charlie Ferster on schedules of reinforcement (Ferster & Skinner, 1957). But the fashion has changed. By contrast with the situation in the early years of our discipline, the present journal's most recent Cumulative Index, covering Volumes 21-40, lists only 12 articles concerned with acquisition.

Back then, the process of extinction also played a central role, both in our extrapolations to everyday behavior and at the theoretical level. For example, Hull had devoted an entire chapter in his *Principles of Behavior* (Hull, 1943) to the topic, and had included "resistance to extinction" (the number of responses) as one of the four dependent variables to be used in testing his system. Although we were not followers of Hull, most of the research published in the journals during that period reflected his orientation and that set a context for our efforts. Similarly, in *The Behavior of Organisms* Skinner (1938) had devoted a large part of his third chapter (pp. 61-115) to the same topic. He treated extinction as a fundamental behavioral process, almost on a par with conditioning itself. The "reflex reserve"—the supply of responses accumulated during reinforcement and expended during extinction—had been the most prominent and most fully developed of his intervening variables. (For a recent evaluation, see Killeen, 1988.) Along with rate of responding, which was not always appropriate, the size of the reserve had served as one of his two measures of "reflex strength." Although Skinner had subsequently abandoned the reserve as a theoretical device (Skinner, 1940) and resistance to extinction as a measure of the strength of the response, he continued to take an interest in the effects of a variety of factors on the

subject's performance under an extinction procedure (e.g., Skinner, 1950). It is small wonder, then, that at Columbia we routinely used the number of responses during extinction to measure the effects of prior training. But today this measure is clearly out of favor.

It may come as something of a shock to those who became familiar with the experimental analysis of behavior only after the present journal was founded, but almost all of the conditioning research during my stay at Columbia had been based on the traditional experimental design in which the mean performance of one group of subjects is compared with the mean performance of another group, treated differently in some way, and a statistical test is conducted to determine whether the results could have arisen by chance. The outstanding exception was Murray Sidman's dissertation (Sidman, 1953b). I was impressed with Murray's dissertation and was soon convinced of the efficacy of the within-subject comparison, but I was nonetheless taken aback when *JEAB* was founded to discover that the stated purpose of the new journal was not to provide a convenient place in which to collect articles on the substantive topics in which we were interested but to provide an outlet for a specific type of experimental design. Being a recalcitrant type, I even pushed my way through two rounds of reviewing to acceptance of a manuscript based on a group design. Then, to make it clear that I was concerned with the principle rather than with my personal fortune, I withdrew the manuscript and published it elsewhere (Dinsmoor, 1958).

Today there is a tendency, I believe, to discount early work in the experimental analysis of behavior, on the grounds that the methodology was not acceptable. I do not agree. If care is taken in averaging (see Estes, 1956; Sidman, 1952) I think that the methodology is legitimate, and to me the questions that were asked were usually closer to the center of the discipline, broader in their implications, simpler, and more directly relevant to everyday concerns, than those addressed in contemporary research. (This may be a widespread phenomenon—see Appley, 1990.) Try them on an undergraduate class. I shall probably be dismissed by some as out of touch with modern thinking, but to me the period while I was at Columbia remains the Golden Age of research on the most elemental of behavioral processes.

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